

THE
PSYCHOLOGICAL BULLETIN

GENERAL REVIEWS AND SUMMARIES.

RECENT LITERATURE ON THE PSYCHOLOGY OF
TESTIMONY.

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In September, 1909, those who were fortunate enough to attend the twentieth anniversary of the opening of Clark University had the pleasure of hearing two lectures upon the psychology of testimony from the pioneer and best-known expositor of this field of activity, Professor William Stern, of the University of Breslau. The abstracts of these lectures (9) may be consulted for a condensed survey of the problems, the methods, the technical results, and the practical consequences for pedagogy and for law of the work that has thus far been done in this field. In general, Stern's survey covers the field in much the same manner as the review of the subject published in this magazine by the present writer several months ago.¹

The most extended direct contribution to the psychology of testimony that has appeared since the review just mentioned is that of Breukink (2) of Utrecht. This investigator employed the regulation picture tests, but made features of the following points: first, the use of a large number (108) of observers in order to compare the relative importance for fidelity of report of the two factors, sex and general culture; and second, the use of a series of three tests, separated by intervals of one week, in order to determine the extent to which repetition with knowledge of errors (the picture was shown again in each instance after the report had been completed) would improve the fidelity of report. The following are the more important conclusions:

¹ 'The Observer as Reporter: a Survey of the Psychology of Testimony,' PSYCHOLOGICAL BULLETIN, May, 1909, Vol. VI., 153-170.

(1) persons of culture (physicians, professors, teachers) give more extended and more accurate reports than do relatively untutored observers (nurses, workingmen, etc.). (2) Men students report slightly more items than do women students, but with less accuracy, particularly when colors are concerned. (3) Practise in reporting increases the reliability of reports, — the improvement being especially noticeable in the depositions and in the answers to suggestive questions. (The resistance to suggestive questions was 75.6, 78.5, and 84 per cent., respectively, in the three tests.) (4) Men offer greater resistance to suggestive questions than do women, but this sex difference is but one-third as great as is the difference between the cultured and the uncultured group. Similarly, there is but little sex difference in the reliability of statements made under oath, but members of the uncultured group took oath to suggestive questions three times as readily as did members of the cultured group. (5) The use of written instead of oral reports apparently tends to increase the number of indefinite answers, but to decrease the number of erroneous answers.

Certain tests introduced by Breukink upon the estimation of temporal and spatial magnitudes led to the following additional conclusions: (6) Short time-intervals (1 min.) are strongly overestimated: intervals of medium length (12 min.) show no overestimation, while longer intervals (35 min.) are oftener underestimated. (7) Women are generally inferior to men in estimating time-intervals, but no marked difference appears in estimating spatial extents. (8) Distances of 2 to 3 m. are subject to slight underestimation; distances of about 20 m. are commonly overestimated, while longer stretches (about 137 m.) are again underestimated. (9) Unless their calling gives them special training, uncultured persons judge distances less accurately than do cultured persons.

Minor contributions to the psychology of testimony are those of Buchholz (3), who relates two incidents illustrative of error in report, and of Schneickert (8), who pleads for instruction in the case of the members of the German police schools in the recognition from verbal descriptions of persons who are sought after by the police.

More activity is to be observed along another line which bears upon the psychology of testimony, viz., the use of the association reaction method in mental diagnosis. On the same occasion as the addresses by Stern there were delivered a series of lectures by Jung (5) which do not directly concern the psychology of testimony, but which are worthy to be mentioned as accounts of the manner in

which the association method has been developed and manipulated by one whose name is most intimately connected with its use.

As regards the application of the method to the detection of information, it will be recalled that, in 1909, accounts of experiments in this field were published by Yerkes and Berry (10) and by Henke and Eddy (4). These experiments have recently been reviewed and criticized by Binet (1), whose comments appear to have been anticipated, in part at least, by the more careful qualitative and quantitative study of the method by Leach and Washburn (6). The general arrangement of the experiment has been similar in all three of these investigations: the subject is allowed privately to open one of two boxes, each of which contains some single object (a watch, a snake, an ink-stand, etc.), and the experimenter endeavors, by means of a prepared list of some 100 terms to which the subject responds as in a regular association test, to determine which box has been opened. Binet's criticisms are in essence these: the association method is more artistic than scientific; its results hinge very largely upon the skill of the experimenter; its fundamental principle—an association that is quicker than another association may be considered a more natural, less reflective, less inhibited association—is sound, but this principle must be subjected to numerous qualifications, *e. g.*, a perfectly natural association may be given slowly; the associations to significant words may be given more rapidly if the subject prepares for the test with sufficient skill in advance; some subjects may take deliberately an attitude of indifference such that all associations have approximately the same speed; finally, it is possible that subjects might conceal their knowledge by deliberately slowing all reactions to times of 3 or 4 sec.

In the more extended and more carefully executed tests of Leach and Washburn, the reliability of the method appears to be sufficiently well demonstrated so far as this particular type of experiment is concerned, for the box that was opened was correctly diagnosed in 52 of 53 trials. An attempt was made also to diagnose the box by reference to the quality (character) alone of the associations. It developed that this method would enable an experimenter to detect the box in a fair number of cases, but that, taken alone, the method is not reliable and is far less serviceable than the method of studying the association times, taken alone. Disregarding the numerous conclusions concerning the use of the quality associations, which are worthy of study by any one who tries the test, the following points may be adduced with regard to the use of the times of the associations: (1) "In all the experiments but four, the average reaction time for words

referring to the object looked at was longer than that for the words referring to the other object." (2) "In rare cases, the dangerous words are reacted to with abnormal quickness." (3) "The longest single reaction time to a relevant word proved, in every series but two, to be that of a reaction to a word referring to the object seen," so that "this record is better than that of the average reaction times" and is "the best single criterion on which to base a decision as to the object seen." (4) "The reaction times to relevant words seem usually, though not invariably, more irregular [larger mean variation] than those for words referring to the other object."

The neat outcome of these more refined laboratory tests does not necessarily guarantee that mental diagnosis by the association reaction method will be of positive service to the jurist, for he will seldom find his conditions so simple and clean cut as those of the alternative box test. This opinion is confirmed in particular by Ritterhaus (7), who declares most emphatically that the method is 'absolutely unfit' for use in criminal procedure. His experiments lead him to assert (1) that a considerable number of persons, especially of women, are so constituted psychically, whether by nature or by circumstances that influence them temporarily, as to render it impossible to apply the method to them successfully, and (2) that, even in the case of those whose mental disposition permits the use of the method, the results, whatever they may be, can never serve unequivocally as bases for legal action.

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SUGGESTION.

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In the papers by Jones (6) and Strong (8) we have two reports upon experiments carried on in the Psychological Laboratory of the University of California. The experiments were attempts to continue the work begun by Brand in the same laboratory some years before. Brand's work was on the reproduction of linear distances as affected by such suggestions as, 'Make short,' 'Make long,' 'Don't make too long,' 'Don't make too short.' He found that to a certain extent his suggestions were successful.

Miss Jones employed the following method: Two white pegs 30 cm. apart were exposed to the subject for $1\frac{1}{2}$ seconds. The subject was then requested to adjust two other pegs so that the distance separating them would be equal to the distance separating the pegs previously exposed. Just preceding each of the exposures one of the following suggestions was given: 'You are now able,' 'You are now unable,' 'I am now able,' 'I am now unable.' The suggestions were spoken by the operator, read by the subject from a printed motto, read by the subject from printed symbols, or spoken aloud by the subject. The purpose of the experiment was to discover to what extent suggestions as to ability and inability would affect the ability to reproduce visual linear distances. The measure of ability is assumed to be the constant error and the variability. The author summarizes her results as follows: "The three subjects showed the same significant tendencies in responding to suggestion; that is, with each, the suggestion of ability produced generally less error and less variability than did the suggestions of inability; and estimates with no suggestion were in each case still nearer the standard" (p. 275).

Strong extended the work of Brand and Jones but substituted the maximum grip of the hand for the reproduction of visual linear distances. The suggestions were, 'Now you can make it stronger than usual,' 'Now you can't make it as strong as usual.' The suggestions were given as in the preceding researches. The results show practically no superiority for the positive suggestion (can) over the negative one (can't). The maximal grip was as great in one case as in the other. To secure a standard with which to judge the results secured from both the positive and negative suggestions Strong interspersed controls by saying 'Now, neutral,' as he handed the dynamometer to the subject for the maximum grip. He designated this his 'neutral

suggestion.' The grips following the positive and the negative suggestions were stronger than those following the neutral. It seemed to make but little difference whether the suggestions were pronounced by the operator, read from a motto, inferred from a sign, or pronounced aloud by the subject.

Both Jones and Strong cautioned their subjects to avoid all tendency voluntarily to follow or resist the suggestions, but merely to pay attention to the suggestions. The subjects supposed that the suggestions were having little or no effect. Great care seems to have been taken in conducting the experiments, the results are well analyzed and discussed and the relation of the present experiments to preceding ones is fully indicated.

The term suggestion is employed in both a wider and a narrower sense. When employed in the wider sense it may include practically every act of perception and of association. In the restricted sense it is used to designate an extreme form of inhibition, a narrowness of consciousness, a dissociation. It thus stands in marked contrast with deliberation and always implies a shunting out of ideas which the ordinary association of ideas would call up. Brand's research was an investigation upon suggestion in the narrower sense. The authors of the articles being reviewed assert that their experiments are a development of Brand's, and yet it seems to the reviewer that these two experimenters have been working upon suggestion in the broader interpretation of the term only. It is desirable to confine the term to the narrower sense in technical experimental work. These researches would lose nothing of value if in every instance some other term were substituted for suggestion. The title of the two articles might then be written: Experiments upon the Reproduction of Distance as Influenced by Distracting References to Ability and Inability; and The Effect of Various Types of Coaching and Taunting upon Muscular Activity.

As is well known, the after-image from white light does not remain one constant color but color after color develops as the after-image fades. The suggestion was given (7) to each of twenty college students that the sequence of the colors was the spectral order although the suggestion was contrary to fact. The suggestion was successful in varying degrees with 17 and failed with 3 students. The same 20 subjects were given the suggestion that a wire was being heated by electricity. This suggestion also was more or less effective with all the subjects. The subjects who were the most suggestible to colors in the after-images were not generally more suggestible to the heat

illusion than were those less suggestible to the color of the after-images. The coefficient of correlation was but little above zero. Both the experiments were experiments upon suggestion in the narrowest possible usage of the term. The experiments were conducted carefully and similar results may be secured by others verifying the results. The conclusion reached is that suggestion is not a simple elementary condition constantly present in the same degree in the same individual. But rather, like perception and memory, it varies from moment to moment and from one content to another. It is not safe to speak of a person as suggestible in general unless his suggestibility has been proven in diverse instances. A single display of a high degree of suggestibility would not indicate a similar degree of suggestibility in general.

The paper by Binet and Simon (3) constitutes Chapter XI of the larger work on *L'Intelligence des Imbéciles*. The authors discovered that imbeciles responded to almost any suggestion if only the desired or expected response were known. They came to the conclusion that such a response indicated nothing as to the conception of the subject. They analyzed suggestion into two classes. The first class affects the ideas and conceptions and results, in extreme forms, in absurd hallucinations. The second class affects only words, acts and attitudes. This latter class of responses is only an apparent indication of suggestion and may indicate nothing more than docility. The imbeciles are extremely suggestible as judged according to this second class but the proof has not yet been established that they are highly suggestible if judged according to the first class. In his article (5) Hyslop takes the ground that suggestion can not be taken as a causal factor in accounting for mental healing. Suggestion is merely the condition under which the healing takes place. Bechterew's work (2) is already known to the readers of the BULLETIN in the German translation made in 1905. Anastay's article (1) cites two instances in which the phonograph had been employed for giving suggestions.

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VALUES.

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Classification continues, naturally, to be one of the chief preoccupations of what, according to Mr. J. S. Moore, is the new philosophical discipline, born in 1909. Mr. Moore's own paper (5) is wholly concerned with the 'system of values.' The point of view of the psychologists and relativists (Orestano, Tawney, Urban), as he is pleased to call them, makes their classifications too limited, since it excludes the transcendental values. In Münsterberg's system, on the other hand, he finds a confusion of the two types. His own classification, starting with the principle of life as an adjustment, recognizes three fundamental types, (1) factual values or values of adaptation, (2) ideal values or values of harmony, (3) transcendental values, or values of perfection.

A quite different point of view is involved in the classification of Mr. J. A. Leighton (3). It is based upon the principle that "a metaphysics of values can be regarded only as a special way of formulating a metaphysics of persons," and that "values *per se*, apart from the attitudes and achievements of selves, have no substantive existence."

Still another point of view underlies the classification in the two articles of M. Fongsegrive (1). There are for him two fundamental classes of values which go back to a fundamental distinction between the pleasures of sense and the pleasures of sentiment. The purely sensible pleasure is situated in time and localized in space. The pleasures of sentiment *durent d'avantage* and are not localized.

This distinction, from which Fongsegrive's whole treatment is developed, brings out immediately the point of interest in an otherwise somewhat lightly handled discussion of the subject, namely, the Bergsonian point of view,—an interest, I may add, somewhat heightened by the use that the social philosophers are beginning to make of Bergson's ideas and by the fact that he himself is about to apply his

'method of intuition' to the field of social philosophy. Fonsegrive accepts Bergson's purely qualitative view of conscious states, and infers that the concept of quantity applies only to economic and social values, the latter being at bottom economic. "All values, in so far as they are social, enter into the commerce of men." And, again, "nothing is easier than to apply to all the types of values the language of arithmetic which is that of economic values. Nothing is more easy, but nothing, perhaps, is more futile" (p. 567). The ethical, æsthetic and religious values belong to the 'deeper self'; they can be appreciated by the method of intuition but cannot be scientifically described. From this conception of the *locus* of values he further infers that any philosophy of values must necessarily be "an organization of our ideas in dependence upon our sensibility, a value being such only when we feel it"; that "it must be individualistic and pragmatic, and therefore have little connection with the edifice of science." To avoid this serious fault the philosophy of values must put itself under the control of ethics. If in doing this it runs the risk of falling into prejudices, it is at the same time able to give to rule or law a basis conformable to value itself.

The specifically psychological contributions to the theory of value continue to come largely from German sources. Meinong has published a new edition of an earlier work (4) for which he has rewritten and enlarged the chapter on *Wertpsychologie*, incorporating the results of recent American treatment of the subject, from a genetic point of view. Schlesinger (6) has sought to apply the methods of experiment to the processes of valuation; and Landmann-Kalischer (2) develops a realistic theory of value on a distinctively psychological basis.

Schlesinger's study of the 'Begriff des Ideals' is in the first place a contribution to the psychology of valuation. Thus "the function of constructing ideals consists in a process of valuation," and again, "every ideal demand is a valuation, although not every valuation is an ideal" (p. 165). The object of his study is "to develop the scheme of an empirical psychology of ideals by a systematic presentation of previous theories of the ideal." Having thus found the possibilities of the ideal experience by the experimental investigation he "determines the actuality or non-actuality of these experiences in individuals" (p. 224).

The method of experimentation employed is that of the *questionary*, supplemented and controlled by direct question and answer. Method and results alike will require, and indeed are worth, con-

scientific criticism, but it has at least been proved possible to extend the methods employed in the psychology of the thinking processes to the processes of valuation and formation of ideals. Nor are the results by any means merely a 'painful elaboration of the obvious,' over-elaborate as some of the analysis seems to be. They coincide at certain significant points with the more novel conclusions of pure analysis in this field, *i. e.*, the relation of valuation to judgment and belief. They have also, one might venture to say, made it incumbent upon any one who would use the concept of ideal, in ethics or elsewhere, with any degree of exactness, to go through this, or a similar analysis, of what the *ideal* experience really is.

Psychological, though in a somewhat different way, is also the article, *Philosophie der Werte*, by Landmann-Kalischer, whose contributions to aesthetics and valuation have been noted in former reports on these subjects. It is in the first place a largely conceived criticism of Münsterberg's *Philosophie der Werte* (a summary of which appears as a review in the same number of the *Archiv*), but this is merely preliminary to a theory of value developed in a psychological and realistic spirit. The critical portion may be summed up in two propositions. (1) Existence, instead of being one of a number of values, is in reality presupposed by all the other values. (2) Of the four worlds of values, logical, ethical, æsthetic and religious, only the first has constitutive importance. The others presuppose it.

The constructive portion, including sections on the psychology of the emotional sphere and on the objectivity of value judgments, has important features which can only be enumerated here. Value and reality, it is held, are given simultaneously in feeling; the dependence of feeling and value upon individuality is asserted; the dependence of the norm upon psychological fact is recognized; feeling is held to be capable of objectification, and upon this objectivity of feeling is based the criterion of objectivity of values. In the development of this doctrine of the objectification of feeling is introduced a modified theory of the reproduction of feeling which is not only important in itself, but interesting also as showing the growing prominence of this conception.

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REACTION TIME.

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The method of the time of reaction is applied by Bourdon (1) to the study of the processes of identification, resemblance, and localization and of combinations of identification and comparison, comparison and identification, comparison and localization, and double identification. Twenty brief series of experiments with colors, areas and forms (numbers) are reported. Reaction time of identification was measured by using two color stimuli (red and green), or two rectangles (20 mm. and 10 mm.), or two numbers (2 and 3), with reaction by the right hand if one stimulus was presented, with the left if the other was presented. The time of perceiving resemblance was measured by presenting stimuli in four pairs, *e. g.*, red-red, green-green, green-red, red-green, with similar arrangements for rectangles and numbers, and reaction made when the stimuli were alike. Localization (direct) was tested by presenting stimuli in two pairs, *e. g.*, red-green, green-red, with reaction with the right hand to red on the right and with the left hand to red on the left. Localization (crossed) was tested with the same arrangement except that reaction was made with the right hand when the stimulus was on the left.

The results show that the time of perceiving resemblance is longer by about 75σ than the times of identification and localization (direct) which both average a little above 300σ. Crossed localization takes about 50σ longer than direct.

A few series of experiments were made in which stimuli were combined in various ways so as to involve more complex combinations of identification and comparison, localization and double identification, with a lengthening of the times in proportion to the degree of complexity. An interesting suggestion is made for the measurement of fatigue by the time of reaction in associations newly formed where the influence of distraction, preoccupation and fatigue is quickly evident.

Kiesow's (2) study of sensory, muscular and mixed types of simple reaction to tactile stimuli, published in 1904, has been carried on with auditory stimuli. Experiments were made on three subjects representing the three types. The average times and mean variations for the three subjects remained relatively constant, *e. g.*, to the more intense stimulus, sensory, 148.7 σ , m.v. 14 σ ; muscular, 117.1 σ , m.v. 12 σ ; mixed, 130.3 σ , m.v. 13 σ .

Experiments were also made, Kiesow himself acting as subject, in which the direction of attention was voluntarily controlled. Sensory reactions to the maximal intensity, 1,000 cases, averaged 172.53 σ , approximating closely the values of Lange and much longer than those of Bergemann (1906). Muscular reactions gave results as follows: 1st 200, 114.84 σ ; 2d 200, 105.33 σ ; 3d 200, 101.68 σ , which results led Kiesow to distinguish three stages of practice in muscular reactions. Mixed or neutral reactions, 200 cases, gave an average of 140.1 σ .

The paper contains interesting observations on differences in reaction times with the various fingers, with right and left hands, with right-handed and left-handed subjects and on the effect of varying intensities on the different types of reaction.

Kiesow and Ponzo (3) give results of preliminary experiments on sensory and muscular reactions to thermal stimuli, both warm and cold (0° to 48°-49° C.). Ingenious apparatus was devised for dropping water on temperature spots of the forearm and for eliminating so far as possible radiation and tactile stimulation as factors in the experiment. Experiments were made on one subject of the mixed type, giving reaction times to cold 192.7 σ and to warm 206.7 σ . Muscular and sensory reactions were made by the two authors with the following average results:

	K.	P.
Sensory	Warm.....231.4 σ	226.3 σ
	Cold.....254.6	259.4
Muscular	Warm.....145.5	140.7
	Cold.....157.4	143.1

The authors give a valuable review of the literature on thermal reactions and call attention to the fact that their results differ from those of all previous investigators. The study is to be carried further.¹

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¹ For reaction times in connection with experiments on the association reaction method see above, p. 366-368.

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RIGHT-HANDEDNESS AND ALLIED TOPICS.

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H. T. Woolley (3) records some observations made on a female infant. No steps were taken to favor the development of right-handedness. In daily life no preference for the use of either hand was noticed until the beginning of the ninth month; at the end of this month the note was made: 'evidently right-handed even to casual observation.' At this time the tendency to right-handedness even overcame an earlier habit of waving good-bye with the left hand, acquired owing to the child being carried in such a way that the left hand was freer. However, exact experimental observations carried out during the seventh month indicated the existence of a preference for the use of the right hand that was not otherwise discernible; this was much more pronounced in the case of actions presenting some difficulty, such as reaching far. The seventh month was the one in which the child began to babble syllables, suggesting a connection between the development of speech and right-handedness. (There is however no evidence that the tendency to right-handedness first appeared in this month, for it was not investigated before.) The writer accepts these observations as supporting the general view that right-handedness is a normal part of physiological development, and is not an occurrence to be explained by training. Gaupp (1), whose brochure in this series, *Ueber die Rechtshändigkeit des Menschen*, constitutes one of the best reviews on the subject of right-handedness, gives here a similarly excellent review of the asymmetries of the normal. It deals with such facts as asymmetry of the face, head, limbs, spinal column, etc. Although these are important considerations for anyone who attempts to deal with the problems of right-handedness, even from a psychological point of view, they are too exclusively anatomical to be suitable for abstraction here.

The present writer publishes (2) a continuation of his researches on allochiria. Dyschiria is a disorder in the synthesis of the sense of 'sidedness' (chirognostic feeling), and has a purely psychogenetic origin, being always a manifestation of hysteria. There are three

stages of it: achiria, in which the subject has absolutely no idea of the side of a part the sensorial acuity of which may be perfectly intact; allochiria, in which the part altogether conveys the opposite sidedness to that normally associated with it; and synchiria, in which it conveys at the same time both. There are two distinct varieties of dyschiria, unilateral and bilateral respectively, and a case of each is related in detail; in the former the affected side seems to be displaced over on to the opposite side, as if it were folded on a vertical axis; in the latter there is no such illusion, the two sides being merely incorrectly named. Special experimental observations showed that in both cases the subject could correctly identify the part touched, provided it was indicated in some other way than by the use of the words right and left. The other circumstances and manifestations led to the following hypothesis being formulated: "The mental processes relating to a given part of the body are divided into (a) the æsthetic group of perceptions, and (b) the autosomatognostic group of memory feelings, of which chirognosis is one. Dyschiria results whenever the second group is dissociated, the first being relatively or absolutely spared, a rare occurrence referred to as the paradoxical type of cleavage." Other questions discussed, such as, for instance, the relation to one another of the three stages of dyschiria, are not here mentioned, being mainly of psychopathological interest.

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CONTRIBUTIONS TO THE LITERATURE OF FATIGUE.

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The most extensive study of the year is that of Weiler (1), who introduces a new instrument for the study of muscular contractions. Weiler has introduced other psychological apparatus reported to be of considerable merit, and the features of this new instrument are well worth noting. The study opens with a brief and mainly theoretical discussion of the terms used in the study of fatigue, and the relation

of *Arbeit* and *Kraft*, this leading to some allusions to the earlier methods of approaching the problem of muscular work. The absolute measure of muscular strength cannot be satisfactorily obtained by present means. The muscle will raise weights which lacerate it, and the maximum resistance of muscle to stretching is affected by too many independently variable factors. Proceeding from the researches of Weber and Helmholtz on the frog muscle, the first researches on the human muscles *in situ* are those of Mosso, whose original ergograph is the starting point of so much of the later fatigue investigation.

There is a brief discussion of some of the previous instruments. While far from complete, the criticisms are excellent for the instruments with which they deal. The defects of the ordinary grip dynamometer, such as the liability to excentric pressure, slipping, and painfulness under extreme exertion, are well recognized. The Chéron-Verdin instrument is better. The author prefers, and it would seem with some reason, a third instrument, whose principle somewhat suggests that of the spring dumb-bells, the two longitudinal halves of which are pressed together during exercise. The apparatus consists of two metal sleeves telescoping into one another against the resistance of strong springs at either end. The length of these metal sleeves corresponds to about the width of the hands, and the circumference can, by supplementary sleeves, be adjusted to the grasp of the hand; an important feature, owing to the different leverages that are otherwise obtained by large and small hands. At one end is a circular, graduated dial, upon which the indicators move as in the ordinary instrument.

A short account of Mosso's ergograph is given, and the modifications introduced by Kraepelin and Trèves are mentioned. Weiler considers that binding or otherwise securing the arm is a considerable source of error through its influence upon the subjective attitude. Another difficulty in the way of standardizing such ergographic experiments is that there exists for each individual a certain optimum weight, and that the immediate comparison of curves made with the same weight does not give a just differential measure of the work-capacity of different individuals. This is one of the troubles that the spring ergograph is intended to obviate, but of these instruments the author describes only that of Binet and Vaschide, not mentioning the form devised by Cattell, or the elaborate apparatus of Bergström. In general, the paper evinces surprisingly little attention to the extensive literature of its subject.

The instrument used by the author is practically that above described, with the addition of a writing device for recording the extent of the successive squeezes upon the dynamometer. The recording principle is that of a small steel wheel moving over copying paper and reproducing its impression upon an ordinary writing surface. There is room upon this surface to record 100 contractions, and within this limit the apparatus is entirely self-contained, there being none of the cumbrous appanages common to so many ergographic methods. The performance in each individual contraction may be read from a specially constructed scale compared with the record, or upon which the record itself may be made. The dial is covered, so that the subject sees nothing of the markings.

The different psychophysical processes involved in the work-curve are next taken up. The viewpoint is here a distinctly physiological one, setting up the psychical function as simply a *Stellvertreter* for the various uncomprehended physiological ones. This question of the individual factors in the work-curve has been thoroughly threshed out in the literature, and this portion of the paper brings up little that is new. There is full recognition of the unanalyzability of several factors in the work-curve, and rather more attention to the emotive factor than is often the case. What must be attempted in the analysis of the curve, is to determine the *Willenskraft* and *Willensspannung* of the subject, the emotive processes, as well as the degree of psychic fatigability and susceptibility to practice, together with the excitability of the motor centers. Only when this has been done do the curves permit conclusions regarding the subject's muscular strength and fatigability.

A relatively small portion of the paper is occupied with an account of experiments. These consisted, with two subjects, of curves of 60 contractions each, one curve being executed on each of ten successive days. On the odd days the work proceeded without intermission, in the rhythm of two seconds, the intervals being marked by a metronome; on the even days a two-minute pause occurred after the thirtieth contraction. This is a type of experiment familiar to readers of the *Arbeiten*, whose special interpretation has been elaborately discussed by Specht. With a third subject, the work was similarly continued for 'over an hour' each day. The results are presented in both tabular and diagrammatic form and worked out in some detail.

The decrease of the curve for 60 contractions is rather steady in rate. When the pause is inserted, recovery is not sufficient to bring the performance up to the initial maximum, contrary to what happens with

the usual five-minute pause in the addition test. Considerable stress is laid on the relation of the two halves of the performance on either side of the pause, and, in general, upon the constant relationship found between immediately succeeding intervals of performance. There is comparatively little practice increase in the gross muscle strength, which the author accounts for on physiological grounds. The most important conclusion stated is that in the execution of muscular work the work-capacity decreases as the logarithm of the work performed, a finding that is reached by processes that the non-mathematical reader does not follow with great facility. In the interpretation of the experiments as regards the relation of fatigability to *Willensspannung*, the variations of the latter, and the general limitations of the method, the paper is not far out of accord with the prevailing attitude on these points.

The study of mental fatigue by Winch (2) is a limited application of a method that has much to recommend it. The procedure was to subject the individuals (pupils in evening schools) to a certain simple psychological test, upon the performance in which they were then classified into two groups of approximately equal efficiency. Upon a succeeding evening the first group was then subjected to another test of similar type, and the second group subjected to the test a fixed period later during the lesson period. Upon the basis of the preliminary test, one group should do about as well as the other; and it is thought that a decrease in the efficiency of the second group should be a measure of the fatigue ensuing in the intervening period. This obviates an often very disturbing factor of practice effect. The author finds a fairly consistent tendency for the later tested group to do more poorly than the earlier one, and reaches the conclusion that evening schools, attended after long hours of labor, are comparatively unprofitable on account of the rapid decrease in mental energy in the pupil during the course of the evening's work.

Dr. Lee's paper (3) is a general review of the problem in its broader aspects. While the term fatigue is sometimes loosely applied to physiological depressions in general, it is more properly used to describe only those depressions essentially due to previous activity. Its sensations are but the subjective signs of its physiological and chemical conditions. There are some excellent reproductions of the muscular fatigue curve, indicating the slowing of relaxation as well as the decrease in height of the curves in cold-blooded animals, and the absence of the slowed relaxation in the warm-blooded. Among the chief chemical conditions of muscular fatigue are the consumption of

oxygen and carbohydrates. Fatigue phenomena may be experimentally produced by elimination of these substances. On the other hand, oxidation and loss of carbohydrate produce in the working muscle carbon dioxid and lactic acid; and a muscle subjected to their influence shows experimentally the same phenomena as of fatigue through work. But in minute quantities, these and such substances, acting as irritants, are the probable cause of the 'warming up' phenomena in mental as well as in muscular work.

The fatigue phenomena of other tissues than the muscles are relatively unstudied. As we should expect biologically, the nervous system appears highly resistant to fatigue. The author is inclined to discount the value of ergographic experiment for determining the relative fatigability of nerve and muscle. The sensations of fatigue probably result from events largely outside the central nervous system, though by no means confined to the peripheral organs that have done the work, since the toxic fatigue substances may be generally distributed by the blood. "The value of training . . .," he says, "consists largely in the development of a power of resistance to the toxic fatigue substances, and is not unlike the production of a condition of tolerance to a poisonous drug by the administration of successively increasing doses of it."

Even the isolated muscle has a slight recoverability from fatigue, owing perhaps to the absorption of oxygen. If the accumulated fatigue substances be partially washed out, recovery is more marked, and still more so if glucose or oxygenated blood be administered. In the complete organism, the value of sleep lies in the more complete elimination of the fatigue substances made possible by the cessation of the grosser bodily activities.

With the theories of a specific fatigue toxin the author does not evince much sympathy.

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READING.

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No comprehensive studies of the psychology of reading have appeared during the year, and Huey's and Dearborn's works remain the latest extended treatments of the subject. Among minor studies that of Dockeray (1) has determined the range of vision for letters of the alphabet by means of the tachistoscopic method.

The average threshold of distinct vision of all the letters of the alphabet for three subjects was 21.5 mm., 22 mm., and 20.3 mm. respectively. These results taken in connection with the accepted results of Dearborn as to the number of fixations in a line of normal reading, indicate that everything which is read comes within the field of distinct vision, and that successive fields may even overlap.

From this investigation it appears that the degree of legibility of the various letters differs with the individual subject, and, that the threshold of legibility and of distinct vision do not coincide for the same individual. In general, the results on this point do not correspond very closely with those of Sanford. The letter 'n,' for example, was found by Sanford to be one of the most legible, but Dockeray finds it to be one of the least legible. The broad letters appear to be usually the most legible, and the short letters and even some of the tall letters the least legible.

Decroly and Degand (2) report some observations upon abnormal children, which emphasize the relative distinctness of various factors of the reading process. Cases are presented which show that words may be recognized without corresponding ability to express the sounds represented.

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PSYCHOLOGICAL LITERATURE.

CALKINS' PSYCHOLOGY.

A First Book in Psychology. MARY WHITON CALKINS. New York: The Macmillan Company, 1910. Pp. xvi + 420.

In this volume Professor Calkins presents an entirely new book, not a revision or abbreviation of the earlier Introduction. The most striking similarity lies in the retention of the plan of giving results alone in the text and putting the discussions of theory and detailed information into the appendix. The text proper occupies but 270 pages, the appendix 150 pages. In many ways the arrangement breaks the continuity of discussion, and much that seems to the reviewer essential to an understanding of the text must be sought in the appendix. The preface asserts that the main change in general standpoint consists in giving over the division of psychology into ideapsychology and self-psychology and in retaining only self-psychology. It seems to the reviewer that the self-psychology has also had some of its more characteristic features softened and that the whole treatment more nearly approximates the discussions of other authors. The self appears only as a center of reference and an auxiliary to avoid the discussion of troublesome phases of problems. If permitted to define the term for one's self no one would object to the use of the self where it is used, and the term is never defined by the author; it is merely an ultimate of our conscious experience.

In order of treatment the work follows a recent suggestion that psychology might be approached more inductively, and after the preliminary discussion of method and general principles turns at once to perception and imagination. The departure from current order is not so great as appears since after a preliminary description of a momentary consciousness explanation turns at once to sensation; the material usually discussed under perception is treated later in the customary place. The description of the momentary consciousness serves as an admirable introduction to the work as a whole, but the order of treatment is after all not different from the traditional ones. The matter is approximately the same as in the generally used texts. More emphasis is laid upon introspection and its results than upon the results of experimental research, but this is to be explained by the elementary character of the text.

The volume perhaps emphasizes description and classification rather more than most recent works. If a process can be referred to a class and given a name it is satisfactorily disposed of even if its conditions and consequences are not stated. The result is the introduction of a relatively large number of ultimates and relatively little attention to practical outcome or origin of processes. Space is an ultimate characteristic of perceptions, attention is an ultimate fact of consciousness, not even a characteristic of mental states, feelings of relation are ultimate as is the feeling of realness that prepares the way for action and belief. Will, it is interesting to note, escapes this pigeon-holing. It is analyzed into relation to the self and a feeling of realness, together with the consciousness of the future and an experience of linkage or connectedness. So far does this tendency to rest content with classification go that the student will get little notion of the conditions of mental activities or of how they may be effectively developed. One is even reminded at times of the entities of the faculty psychology, although it is evidently far from the purpose of the author to revive faculties.

The style of the book is delightful and it is to be recommended most warmly to the general reader. The work vies with the novel in the vividness and circumstantiality of its portrayal of mental life. Every point is aptly illustrated from life or from a wide range of literature. It might be questioned if the steps in the arguments are sufficiently distinguished for the student. But any criticism of a book from the student side is dangerous before trial, and certainly for any other than the student the literary treatment is above reproach.

An occasional debatable statement has been allowed to creep in. Thus it is asserted that the motor area in the cortex is both forward and back of the Fissure of Rolando instead of in front only as seems to be agreed by neurologists. One might question too whether negative judgments are always analytic. Certainly it seems that anything that may be asserted might also be denied. These defects are too few to impair seriously the value of the work as a text. Undoubtedly the work will make a satisfactory text for the elementary student of general interests, although to the reviewer it is a matter of regret that more of the results of experimental work have not been introduced, and that the treatment should have stopped short of an analysis of many of the experiences that are accepted as ultimate.

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MEMORY AND ASSOCIATION.

Über das Zusammenwirken verschiedener Sinnesgebiete bei Gedächtnisleistungen. ALFRED VON SYBEL. Zeitsch. f. Psychol., Vol. 53, pp. 258-353.

This report is a link in the series of papers and monographs upon memorizing which have issued from the laboratory of Professor G. E. Müller. The reader will remember that the interest of the Göttingen experimenters centered first in refining, elaborating and exploiting the memory-test methods of complete memorizing, of right associates and of retained members. He may have noted that in the last few years their interest seems gradually to have shifted from the experimenter's method of procedure to the subject's method of memorizing in its individual peculiarities. The present paper deals with the interaction of different senses in memorizing. It compares the results of presenting material to the eye and to the ear of learners of different image-types when they were required and when they were not required to articulate the syllables and thus to traverse the series in kinæsthetic as well as in visual or auditory terms. The material consisted for the most part of 'normal series' of nonsense syllables. The procedures were as follows: (1) A visual-kinæsthetic-auditory procedure in which the subject was required to read the series aloud from the kymograph; (2) a visual-kinæsthetic procedure in which he read the series with noiseless articulation; (3) a strictly visual procedure in which he was required to suppress all speech-movements; (4) a primarily visual procedure in which he might do as he liked in the matter of articulation, provided that his behavior was uniform; (5) a visual-auditory procedure in which the experimenter pronounced each syllable at the instant in which it met the subject's eye and in which the subject was ordinarily allowed to articulate silently but not to read aloud; and (6) an auditory procedure in which the experimenter alone read the series but which resembled the visual-auditory in other respects. It must be noted that by no means all of these procedures were used with all the seventeen subjects. The general experimental method was a combination of the methods of complete memorizing and of right associates. When, during a round of presentations, the subject believed that he could recite the series, he signaled with his hand to the experimenter, who stopped the kymograph, timed the recitation, and recorded the number of presentations necessary. The effect of different rates was tested with different procedures. The series were read in trochaic rhythm. The test-proce-

dure of the method of right associates ordinarily began five minutes after the end of the subject's recitation. The accented syllables were presented in part to the eye and in part to the ear.

Of the writer's findings, the following seem most important: (1) No real criterion of a subject's image-type is furnished either by the number of repetitions necessary for learning with the different procedures or by his remarks upon the procedures. The objective results were lamentably alike for subjects of all image-types. (2) As regards the number of repetitions necessary for learning, the visual-kinæsthetic-auditory, procedure, the visual-kinæsthetic, the visual-auditory and the auditory all have an advantage over the silent, primarily visual procedure. Among the advantages in articulating the syllables may be the fostering of kinæsthetic persistence and the formation of kinæsthetic associations, as well as the steady attention and exact apprehension of the syllables which are involved. On the other hand, the sound of the subject's or of the experimenter's voice in pronouncing the syllables seems to make the impressions more penetrating. The advantage of the auditory procedure is surprising in view of the objections expressed by subjects of all types. (3) As regards the number of right associates scored, the case is reversed: the primarily visual procedure has the advantage over all the others just named. This fact falls under the general law that those series are better remembered which, on any account, are learned with greater difficulty. (4) According to the introspection of the subjects, the method of learning becomes less visual and more kinæsthetic when the series are hard to learn, — when, for example, the subject is tired or the series are rapidly presented. In this connection, it should be noted that the advantage in the subject's reading aloud is more marked when the rate of presentation is quick. The paper abounds in interesting bits of introspection which cannot be given here.

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Beiträge zur Assoziationslehre auf Grund von Massenversuchen.

FERDINAND REINHOLD. *Zeitsch. f. Psychol.*, Vol. 54, pp. 183-214.

This paper is based upon association-experiments made by the method of chance word-reaction upon groups of school children (girls), taken, thirty each, from ten successive grades and ranging in age from seven to seventeen years. Toward the formation of such an 'association-lexicon' as Saling compiled, the writer contributes the

first reactions to forty-six stimulus-words. He interests himself chiefly in the nature of the most common reactions and in the degree to which individual subjects tend to react with the words most common in their group. With the aid of some collateral experiments, he is enabled to draw the following conclusions: (1) In each group of reactions to a given stimulus-word, some one word predominates in such a marked degree over others that the favorite reaction is not at all hard to determine. (2) In the matter of logical relation between the stimulus-word and the favorite reaction-word (the relations of part to whole and of whole to part, for instance), so much difference exists between different stimulus-words that the list of words actually used in any study of such relations is all important and should never be suppressed. (3) Roughly speaking, younger children show greater individual differences in their reactions than older children. (4) In children, at least, a tendency toward extraordinary reactions is not a sign of superior intelligence, nor is a tendency toward associations by similarity of sound a symptom of inferior intelligence.

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TASTE SENSATIONS.

Sensations gustatives. LANGUIER DES BANCELS. *Année psychol.*, Vol. 15, 273-299.

This paper is not a report of the writer's experimental research, but is a clear, concise and excellent review of our present knowledge upon the subject of taste. The writer discusses (1) the number of taste-qualities; (2) taste-stimuli and the ion-theory of their nature, summarizing the results of Höher and Kiesow and of Herlitzka; (3) the anatomy of taste, considering the gustatory field, nasal taste, so-called, and the vexed question of the paths taken by the gustatory nerve-fibers to the brain; (4) the functioning of the sense, touching upon the sensitiveness of the different papillæ to the several qualities, upon taste-mixture, contrast-effects and adaptation-phenomena; and (5) such psychometric results as have been so far obtained.

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ORGANIC SENSATIONS.

Ueber die Sensibilität der inneren Organe. E. BECHER. *Zeit. f. Psychol.*, 1908, XLIX., 341-373.

An article designed to supplement, says the author, an earlier paper

by Meumann.¹ Imbedded in a general discussion of the whole question of organic sensation (into which I shall not go) appear the following records of actual experiments on the esophagus and the stomach. A subject was found that could easily swallow and retain one end of a smooth rubber tube, 5 mm. in outer, 2 mm. in inner diameter and 1 m. long. In the other end was inserted a glass tube from the end of which a three-branch Y tube of hard rubber projected, two of the branches being further connected each with a rubber bulb, one bulb containing water at 50°-52° C. and the other at 4°-12° C. Temperature stimuli could thus be directly applied to the stomach and, by partially withdrawing the long tube, at various levels of the esophagus. For pressure a bulb of thin rubber was fixed to one end of the tube. This also Herr W. was 'leicht imstande zu verschlucken.' By pressing a bulb at the other end, pressure could be applied at different levels of the esophagus. The subject further submitted to electrical stimuli, given by means of a small piece of tin foil fastened over the lower end of the tube and connected with transmitting wires in the tube.

Becher's results show that warmth and cold, touch and pressure and (from electrical stimulus) a type of sensation without adequate analogy could be felt along the entire esophagus. The nearest description obtained of the effect of electrical stimulus was, for the upper part of the esophagus, its characteristic 'Schwirren' but, for the lower part, a weak burning or caustic quality quite sui generis. These sensations, as well as pressure, were tolerably well localized. Increase in intensity of either caused pain. Becher could not, however, secure any evidence of sensibility in the stomach itself to any of the foregoing stimuli.

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Organic Sensation. ELSIE MURRAY. Amer. Jour. of Psychol., 1909, XX., 386-446.

This is a general review of the whole question of organic sensation, to which, however, are added a few experiments, aiming chiefly to disentangle, through introspection, the organic components in sensory and affectional complexes resulting from various stimuli — taste, odor, visual, cutaneous and auditory. The general results were that the differentiation of internal from external sensation is very difficult, and that internal sensation may, indeed, 'differ from external texturally rather than qualitatively.'

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¹Meumann, E., 'Zur Frage der Sensibilität der inneren Organe,' *Archiv f. d. ges. Psychol.*, 1907, IX., 26-62.

DISCUSSION.

THE 'PENDULAR WHIPLASH ILLUSION.'

The 'pendular whiplash illusion,' as Carr christened the phenomenon, was first described by me some years ago as evidence of the lack of kinæsthetic factors from pursuit eye movements in the visual perception of motion. It seemed to me an unusually clear and unequivocal experimental test, in a field where experimental isolation of the various supposed factors is difficult. Since it was first described the phenomenon has been subjected to critical analysis by two independent investigators. The bearing of these studies on the main theoretical problem of the participation of the eye movements in the visual perception of motion is the subject of the present discussion.

For convenience my original observation¹ may be summarized as follows:

Each arm of a counterbalanced second pendulum bore an adjustable faintly glowing object. If these objects were equidistant from the axis of the pendulum they moved through equal distances at each oscillation of the pendulum. But if either object is visually fixated, in an otherwise darkened room, (1) the non-fixated object appeared to move a greater distance than the fixated, and (2) at the end of each swing the non-fixated object seemed to make an independent little excursion after the fixated object had apparently come to rest, as though it were attached to the latter by a flexible rod. At that part of the swing where photographic registration of my pursuit movements showed the most satisfactory pursuit, the fixated object seemed to stand still, while the non-fixated object seemed to move faster than it should. The adequate pursuit movement neither mediated a perception of motion for the fixated object, nor corrected the exaggerated data from the displacement of the retinal image of the non-fixated point.

In order to rescue the traditional kinæsthetic factor from the 'inevitable logic of the situation' Carr² endeavored to find some other explanation of the phenomenon. In some vital points the observations of Carr's subjects were diametrically opposed to mine. They found

¹ *PSYCHOLOGICAL REVIEW*, Vol. XI., 1904, pp. 13-14.

² *PSYCHOLOGICAL REVIEW*, Vol. XIV., 1907, pp. 169-180.

no difference in the apparent extent of movements of the fixated and non-fixated point. They found little if any apparent movement of the non-fixated object after the other had apparently come to rest. The whiplash illusion is explained by Carr as a function of the positive after-image.

Personally, I have always felt that, from the standpoint of scientific method, Carr's treatment of the experiment was improper. Using relatively bright objects, which gave clearly distinguishable after-images, he discovered after-images, whose manner of fading seemed to him to explain the phenomenon. This always looked to me, to put it bluntly, like salting the experiment. It would certainly have been good procedure to have asked whether the phenomenon disappeared or not when, other factors remaining the same, the luminosity of the objects was so reduced as to eliminate perceptible after-images. Unfortunately, the only instance in which Carr used the method of difference with respect to the after-images involved the introduction of a fixed diffused light within the arc of movement. The loss of the illusion under these circumstances is demonstrably related to the introduction of fixed fields of reference. It was practically equivalent to performing the experiment in daylight.

I feel, however, that I am not altogether blameless in the introduction of after-images by Carr. My account of the experiment was exceedingly brief, and contained no description of my apparatus. When the paper was first read, I demonstrated the apparatus, and failed to insert the details for printing.

As a matter of fact my illumination was of low intensity. The objects were the glowing ends of Fourth of July slow matches, with ash accumulation, viewed from the side. They require partial dark adaptation before they can be satisfactorily followed at a distance of 20 ft. In my experiments there was no distinguishable after-image streak. For obvious reasons I was not particularly proud of my device.

Ford's reexamination of the phenomenon¹ was doubly welcome. He not only devised a better and more flexible apparatus, but he also supplied the methodological deficiencies of Carr's paper. After careful elimination of the after-image effects he finds that the whiplash illusion persists.

It is further noteworthy that Ford, in direct opposition to Carr, entirely confirms the observation that the non-fixated object appears to move a greater distance than the fixated.

¹ *PSYCHOLOGICAL REVIEW*, Vol. XVII., 1910, pp. 192-204.

This gross discrepancy is difficult to explain satisfactorily. It would almost seem that Carr was dealing with a different phenomenon. This may be the case. Personally, I have found it impossible to shift the attention as Carr did, without at the same time influencing the pursuit of the supposedly fixated object. Persistent attention to the non-fixated object completely interfered with normal rhythm of pursuit.

Ford's carefully controlled experiments showed that one might fixate a motionless point and attend to a moving object without producing significant changes in the fixation. This, however, is a normal process constantly recurring in normal life. Its usefulness I have discussed elsewhere in connection with the value of peripheral vision and the long ocular reaction time. Pursuit movements on the contrary are a relatively complex reaction process, demanding delicate adjustment to the precise conditions that each new occasion presents. Relatively slight interruption in the vision of the moving object completely destroyed their rhythm.¹ If during pursuit, one persistently attends to an object that is moving in the other direction, the motive for pursuit is lost, and the eye tends to revert to more primitive and more satisfactory visual conditions.

There is no infallible connection between a moving object and adequate pursuit. On the contrary, the requirement to judge whether an object is moving or not normally leads to the abandonment of pursuit. This fact in itself is evidence of the inadequacy of kinæsthetic data from the moving eye. In the present experiment, moreover, it tends to invalidate the main experimental condition — *i. e.*, adequate pursuit movements.

Confirmatory evidence of Carr's failure to appreciate the delicacy of the mechanism with which he was experimenting, is his tacit assumption that pursuit movements are equally satisfactory for different persons in different stages of practice as well as under different conditions of attention. While the curves of my own eye movements show that, after the pursuit is well under way, the last quarter of each swing is remarkably free from corrective movements,² records from a considerable number of subjects show great variability in this respect especially in the initial stages of the pursuit. The question of the influence of the dark room on the individual variations has not been investigated.

¹ MONOGRAPH SUPPLEMENTS OF THE PSYCHOLOGICAL REVIEW, Vol. VIII., p. 17. Plate I., Fig. 6.

² MONOGRAPH SUPPLEMENTS OF THE PSYCHOLOGICAL REVIEW, Vol. VIII., p. 117. Plate I, Fig. 5.

Brain, Vol. XXXI., p. 474, Plate II.

While normal individual differences could scarcely account for the differences between the observations of Carr's subjects and those of Ford's subjects and my own, they do show the futility of basing a quantitative evaluation of his experiments on records of my eye movements, as Carr attempts to do.

They further suggest the possibility that, relying on the supposed constancy and uniform adequacy of the pursuit movements, Carr may have assumed the existence of pursuit movements which were not present. There is no mention of special training of his subjects or of any objective controls.

It may seem unpardonable to suggest that an observer of Carr's thoroughness performed his experiments without due guarantee of the main experimental condition, but we ordinarily have absurdly inadequate introspective evidence of what our eyes are doing; and the gross differences in his findings, apart from the accurately described after-images, would be entirely explained by natural lapses of the pursuit.

Ford's systematic study of the conditions governing the illusion develops several interesting facts:

1. The illusion is more pronounced when the swings are short and the lights are relatively close together. My own experience leads me to believe that this is not an intrinsic limitation, but depends on the practice of the subject in developing pursuit movements. Under natural conditions long pursuit movements are never provided for by the eye muscles alone. Head and body movements regularly tend to keep the eyes within a few degrees of their primary position.

2. The illusion is found to depend on the law of prior entry of attention. While the influence of this factor is apparently relatively slight, Ford's well controlled experiments leave no doubt that it exists. Personally I have been unable to duplicate his experiments without evidence of lapsed fixation. But the difficulties of registration and even of direct observation under dark room conditions have prevented any adequate test of the correlation of the lapsed fixations and the apparent differences in the swings.

3. Ford's discovery that the whole phenomenon is observable in lesser degree if the non-fixated light is stationary at the axis of the pendulum is abundantly confirmed by my own observations. To my mind this is the most convincing evidence yet produced that the whip-lash illusion is in reality a product of the pursuit movements. It is a perfect use of the canon of difference. If the fixed light is fixated the movement of the swinging light appears perfectly normal, though in my case there is a persistent tendency of the fixed light to sway slightly

in the opposite direction. If, on the other hand, the moving light is fixated by pursuit movements, the illusion flashes out with the greatest regularity.

4. The marked disturbance of the phenomenon by the interposition of a fixed grating is also indisputable. The same effect seems to be produced by any stimulation of relatively larger area, which functions as a fixed object of reference.

Ford concludes that, under conditions which eliminate the after-image effects, the illusion is explainable in some degree by the law of prior entry of the object attended to, but the chief factor in the illusion is "the lower threshold of movement perception by means of displacement of the retinal image as compared with the threshold of movement as perceived by movements of the eyes."

I doubt if the return swing of the whiplash excursion can ever be accounted for by the law of prior entry. With the threshold theory, as elaborated by Ford, I am in entire agreement. Supplementing that theory, however, I must insist that there is no evidence that the "threshold of movements as perceived by movements of the eyes" is anywhere within limits that would make kinæsthetic data serviceable or reliable in the visual perception of motion. On the contrary, while we may admit that kinæsthetic data alone might under some circumstances mediate a judgment of position and the consequent judgment that movement had occurred, the available facts of normal and abnormal nystagmus, of dizziness, Carr's second type of the autokinetic illusion, together with all our direct experimental evidence, support my previous contention that the eye movements yield no significant data in the visual perception of motion.

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A CORRECTION.

While on the whole I consider Mr. Sidis' reply (PSYCHOL. BULL., Sept. 15) to my note on the so-called Psycho-galvanic Phenomenon (*idem*, May 15) as a valuable supplement to the said note, materially strengthening several of my points, one feature of the reply calls for a word of correction. Mr. Sidis seems to imply (no doubt unintentionally) that I deny the fact of the galvanometric deflection under the conditions of his experiment. Of course I intended to make no such denial, but simply to express my strong doubt of the interpretation Mr. Sidis places on the deflection. I do not even deny that other experimenters have obtained deflections without specific muscular activity.

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- A Beginner's History of Philosophy.* Vol. I. HERBERT ERNEST CUSHMAN. Boston, New York, Chicago: Houghton Mifflin Co., 1910. Pp. xx + 406. \$1.60
- The Judgment of Difference.* WARNER BROWN. Berkeley: The University Press, 1910. Pp. 71.
- Psyche's Task.* J. G. FRAZER. London: Macmillan and Co., 1910. Pp. ix + 84.
- World Corporation.* KING C. GILLETTE. Boston: The New England News Co., 1910. Pp. vi + 240.

NOTES AND NEWS.

THE regent of the National University of Mexico and the director of the national school for higher studies have announced the organization of a course in Psychosociology to be in charge of Professor J. Mark Baldwin. A two years' program is indicated and an official invitation to attend the course has been extended to all persons who can comply with the requirements for enrollment, whether or not they reside in the City of Mexico.

PROFESSOR HUGO MÜNSTERBERG has been made director of the newly founded 'Amerika-Institut' during the year of his residence in Berlin as Harvard exchange professor. The avowed aim of the Institut is "the systematic furthering of the cultural relations between Germany and the United States. Strictly political and commercial affairs lie outside of its realm, but everything which refers to education and scholarship, to literature and art, to technique and social welfare, to travel and public interests, to peace and international understanding, will be the fit object of its efforts."

MRS. HELEN THOMPSON WOOLLEY is at present assisting in the work of the department of philosophy of the University of Cincinnati.

THE annual meeting of the American Psychological Association will be held in Minneapolis December 28-30.

